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Cooperation preferences and framing effects

Aurélie Dariel

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Schanzeneckstrasse 1
Postfach 8573
CH-3001 Bern, Switzerland
http://www.vwi.unibe.ch

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Aurélie Dariel†

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Abstract

This paper presents the results from an experiment investigating whether framing affects the elicitation and predictive power of preferences for cooperation, i.e., the willingness to cooperate with others. Cooperation preferences are elicited in three treatments using the method of Fischbacher, Gächter and Fehr (2001). The treatments vary two features of their method: the *sequence* and *order* in which the contributions of other group members are presented. The predictive power of the elicited preferences is evaluated in a one-shot and a finitely-repeated public-good game. I find that the order in which the contributions of others are presented, by and large, has no impact on the elicited preferences and their predictive power. In contrast, presenting the contributions of others in a sequence has a pronounced effect on the elicited preferences and reduces substantially their predictive power. Overall, elicited preferences are more accurate at predicting behavior when others' contributions are presented simultaneously and in ascending order, like in Fischbacher, Gächter and Fehr (2001).

JEL codes: C91, H41

Keywords: public-good game, strategy method, predictive power, framing effect

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†Department of Economics, University of Bern and Department of Economics, Maastricht University; email: aurelie.dariel@vwi.unibe.ch

1 Introduction

This paper presents the results from a laboratory experiment investigating the sensitivity of cooperation preferences to framing effects. Cooperation preferences can be defined as the willingness to cooperate with others when private and group interest are at odds (Fischbacher and Gächter, 2010). Evidence from laboratory experiments indicates that, while some people are unwilling to cooperate with others, many individuals are willing to cooperate provided that others in their group do the same (e.g., Fischbacher, Gächter and Fehr, 2001; Kocher et al., 2008; Rustagi, Engel and Kosfeld, 2010; Thöni, Tyran and Wengström, 2012). At the same time, there is also evidence that framing affects cooperation in public-good games and other social dilemmas. Despite this, it remains unknown whether cooperation *preferences* are sensitive to framing effects.

A framing effect is said to occur when seemingly superficial changes in the presentation of a task affect behavior without affecting material incentives. For example, Andreoni (1995) finds that contributions in a public-good game are considerably lower when the experimental instructions emphasize the negative externality imposed to others by not contributing to the public good, than when they highlight the positive externality generated by contributions. Similarly, individuals are more likely to cooperate with others when the prisoner's dilemma is called the "Community Game" than when it is called the "Wall Street Game" (Kay and Ross, 2003; Liberman et al., 2004; Ross and Ward, 1996) or the "Stock Market Game" (Ellingsen et al., 2012).[1]

Recent studies have argued that framing affects cooperation not by changing preferences, but by changing beliefs about the actions of others (Dufwenberg et al. 2011; Ellingsen et al., 2012; Nikiforakis, 2010). However, none of the aforementioned studies has elicited individuals' preferences for cooperation. Framing has been known to affect choices in non-strategic environments where beliefs about the actions of others should play no role, such as when choosing a lottery (e.g., Kahneman & Tversky, 1986). Therefore, it remains an open question whether cooperation preferences are unaffected by framing.

To elicit cooperation preferences in the present experiment I use the method of Fischbacher, Gächter and Fehr (2001; henceforth, FGF). This method has been used in a number of studies, including Fischbacher and Gächter (2010) who showed that the elicited preferences for cooperation are positively correlated with contributions in a finitely-repeated public-good game.[2] Participants

[1]Other studies that find framing effects in social dilemmas are Bougherara, Denant-Boemont, Masclet (2011), Brandts and Schwieren (2009), Brewer and Kramer (1986), Cookson (2000) , Cubitt et al., (2011), Fosgaard, Garn Hansen and Wengström (2011), Kotani, Managi, Tanaka (2008), McCusker and Carnevale (1995), McDaniel and Sistrunk (1991), Nikiforakis (2010), Park (2000), Rege and Telle (2004), Sell and Son (1997), Sonnemans et al. (1998), van Dijk and Wilke (2000), and Willinger and Zielgelmeyer (1999).

[2]For other studies using this method see Burlando and Guala (2005), Cheung (2012), Herrmann and Thöni (2009), Kocher et al., (2008), Muller, Sefton, Steinberg, Vesterlund (2008), Rustagi, Engel and Kosfeld (2010),

in the experiment are asked in an incentive-compatible way to state how much they are willing to contribute to a public account given the average contribution of the others' group members. The elicited contribution schedules reflect a subject's cooperation preferences. The findings indicate that the majority of participants provides monotonic and increasing contribution schedules. That is, they are willing to contribute more as the average contribution of others increases.

In the present study, framing takes the form of altering the way in which the strategy method is administered. Two features of the FGF method, as it has been used so far, are that the possible contributions of others are presented (*i*) simultaneously in a table, and (*ii*) in an order (i.e., 0, 1, 2, ... 20). These features, however, could affect the elicited preferences for cooperation. For example, the ordering of others' contributions could serve as a cue for subjects to condition their contribution on that of others. Similarly, the simultaneous frame in the strategy method may place subjects in a "cold" state when making their decisions (e.g., Brandts and Charness, 2000; Brosig, Weimann and Yang, 2003; Gueth, Huck, and Mueller, 2001), while the sequential frame may place them in a state that is more similar to that in the standard public-good game. At the same time, however, ordering and sequentiality could affect the level of noise in the elicitation of cooperation preferences and affect its predictive power.[3]

The experiment consists of three parts. In the first part, I elicit subjects' cooperation preferences using the FGF method under three different frames. In the *CONTROL* treatment, all possible contributions of others are presented simultaneously in a table, but, unlike FGF, in a random order. In the second treatment (*ORDERED*), as in FGF, the possible contributions of others are presented simultaneously and in an ascending order. In the third treatment (*SEQUENTIAL*), the possible contributions of the other group members are presented in the same random order as in the *CONTROL* treatment, but sequentially, one by one. In the second and third part of the experiment, I evaluate the predictive power of the elicited cooperation preferences in a one-shot and a finitely-repeated public-good game in which subjects are rematched in every period.

In a recent article, Levy-Garboua, Maafi, Masclet and Terracol (2012) elicit subjects' risk preferences using the Holt and Laury (2002) method under a variety of frames and find that both the ordering of options as well as whether choices are made simultaneously or sequentially affect the consistency of choices and the extent of risk aversion.[4] These findings suggest that the

Thöni, Tyran and Wengström (2009), and Volk, Thöni and Ruigrok (2012).

[3] For example, Fischbacher and Gächter (2010) classify 10 percent of their subjects as "confused" if they could not be classified as either selfish, conditionally cooperative or triangular. The authors report that contributions in the finitely-repeated game of confused subjects were not well predicted by their contribution schedules.

[4] A subject is said to be inconsistent in Levy-Garboua et al. (2012) when their preferences cannot be explained with a standard utility function. The authors find that the rate of inconsistent choices and the level of risk aversion are higher when choices are made sequentially, and when choices are not orderred. Levy-Garboua et al. (2012) do not test the predictive power of the elicited risk preferences.

number of participants with non-monotonic elicited cooperation preferences may be higher in the *SEQUENTIAL* relative to the *CONTROL* treatment but lower in the *ORDERED* compared to the *CONTROL* treatment.[5]. However, it is unclear under which condition the elicited preferences will be better predictors of actual contributions in the public-good game. For example, although the extent of noise may be greater in the *SEQUENTIAL* relative to the *CONTROL* treatment, contribution schedules may still be better predictors of behavior, if the sequential frame places individuals in a similar (hot) state as in the one-shot and finitely-repeated game.

The experimental results indicate that this is not the case. The predictive power of the contribution schedules is higher in *CONTROL* than in *SEQUENTIAL*, both in the one-shot and in the finitely-repeated game. This is partly due to the high number of individuals providing non-monotonic contribution schedules (90 percent of all subjects in this treatment). In contrast, the ordering of others' contributions, by and large, does not affect the predictive power of the elicited schedules. This is the case, despite the fact that, similar to Levy-Garboua et al. (2012), the rate of participants' with non-monotonic contribution schedules is higher in *CONTROL* (50 percent) than in *ORDERED* (30 percent). In general, the frames affect neither contributions nor beliefs in the first two parts of the experiment, but some differences are observed in the third part in *SEQUENTIAL*. I conclude that the simultaneous presentation of others' contributions in an order are appealing features of the FGF method.

The paper proceeds as follows. In the next section, I present the experimental design in detail. In section 3, I discuss the experimental results, while section 4 concludes.

2 The experiment

The experiment consists of three parts. The existence of the three parts is public knowledge, but participants are not informed about the content of each part in advance. In the first part, I elicit participants' contribution schedules using variants of the FGF strategy method. In the second part, participants play a one-shot public-good game, and in the third part, they play a finitely-repeated version of the game. The experiment consists of three treatments. The treatment manipulation occurs only in the first part of the experiment. The second and third part are identical across treatments.

This section begins by presenting the basic public-good game. This is followed by a detailed presentation of each of the three parts and the experimental treatments. The section concludes with a discussion of the experimental procedures.

[5] As I discuss later in the paper, non-monotonic contribution schedules cannot be explained by most standard models used to explain behavior in social dilemmas.

2.1 The public-good game

Participants are randomly divided into groups of three players. Each group member is given an endowment of 20 tokens and has to decide how to divide them between a private and a public account. The payoff function for each group member i is:

$$\pi_i = 20 - g_i + 0.5 \sum_{j=1}^{3} g_j,$$

where $g_i \in \{0, 1, ...20\}$ is the contribution of individual i to the public account. The marginal return of the public account is 0.5, i.e., contributing 1 token to the public account yields a private return of 0.5. Therefore, if an individual wishes to maximize his/her material payoff, s/he should not contribute to the public account. However, since there are three individuals in the group, each token contributed to the public account increases group earnings by 1.5. Therefore, there is a tension between private and group interest.

In the one-shot public-good game, individuals wishing to maximize their material payoff have a dominant strategy to contribute zero to the public account. However, as mentioned, this prediction fails to account for the fact that many subjects contribute positive amounts to the public account, and the fact that many of them are classified as "conditional cooperators", i.e. individuals who contribute if they believe others do so. Fehr and Schmidt (1999, Proposition IV) show that if some group members dislike inequality in material payoffs sufficiently, then positive contributions can be sustained in equilibrium in the public-good game. The authors also show that the contribution of inequality-averse individuals will increase monotonically with the average contribution of other players in their group. Therefore, the model of Fehr and Schmidt (1999) provides an explanation for conditional cooperation in public-good experiments.[6] The intuition is that the inequality-averse individuals will suffer from the inequality when contributing less than the others. Similar predictions are obtained using the model of Bolton and Ockenfels (2000).

2.2 Eliciting contribution schedules and beliefs

In the first part of the experiment, I employ the method of Fischbacher, Gächter, Fehr (2001) for eliciting contribution schedules in the public-good game. Participants are randomly matched to form an anonymous group of three players and told that they will have to make three kinds of decisions. First, they have to decide on an *unconditional contribution* to the public account.

[6] Note that the Fehr-Schmidt model cannot readily explain conditional cooperation in the experiment of FGF (or the one in this paper) as subjects are allowed to condition their behavior only on the average contribution of their peers and not on the existence or the extent of inequality in earnings.

Second, they have to decide how much they are willing to contribute for each possible (rounded) average contribution of the other two members of his group (0,1,...20). I will refer to this set of 21 decisions as the *contribution schedule.* Third, participants have to estimate the (rounded) average unconditional contribution of the other two group members.

The three treatments differ only with regards to the way in which the contribution schedule is elicited. In the *CONTROL* treatment, the possible contributions of the other group members are presented simultaneously, i.e, in a contribution table, but in a random order.[7] In the *ORDERED* treatment, all possible contributions by the other group members are presented simultaneously in a table, in an ascending order. That is, the *ORDERED* treatment replicates the design of FGF. In the *SEQUENTIAL* treatment, the ordering of others' contributions was the same as in the *CONTROL* treatment, but others' contributions were presented sequentially, in 21 successive screens.

All decisions are incentive compatible. In particular, after all participants made their decisions, two subjects in each group were randomly selected and their unconditional contribution was the one relevant for determining their contribution to the public account. For the third subject, the contribution schedule determined their contribution to the public account. In particular, their contribution was chosen based on their contribution schedule and the average unconditional contribution of the other two group members. To incentivize participants to thruthfully reveal their beliefs, they are told that they will receive 3 tokens for stating a belief that exactly matches the average contribution of the other two group members. If their belief is within +/-1 of the average, they will receive 2 tokens. If their estimate is within +/-2 of the average they will receive 1 token. Otherwise, they will not receive any additional tokens.

2.3 The one-shot public-good game

In the second part of the experiment, participants are informed that they will be placed in a new group and that they will play a one-shot public-good game. The one-shot game allows to analyze players' individual responses in a one-shot interaction without repetition effect and learning.

In order to evaluate the predictive power of the individuals' contribution schedule, I also elicite their beliefs about the average contribution of the other group members. Furthermore, to avoid choices in the strategy method affecting decisions in the one-shot game, participants do not receive feedback about the outcomes of the first part of the experiment.

[7] The order was determined by placing 21 numbered pieces of paper in a basket and picking them out sequentially and without replacement before the start of the first experimental session. The order was kept constant in subsequent sessions.

2.4 The finitely-repeated public-good game

At the start of the third and final part of the experiment, participants are informed that they will play the public-good game for ten periods and that the composition of their group would be randomly determined at the start of each period. I note that, following Fischbacher and Gächter (2010), before playing the finitely-repeated game, they are informed about their earnings from the first and second part of the experiment, and the average contributions of their fellow group members. In each period, participants have to decide how much to contribute to the public account and, in addition, provide an estimate of how much they believe the other two group members would contribute on average. Participants receive feedback similar to that in the one-shot game at the end of each period.

2.5 Procedures

The experiment was conducted at the University of Zurich using Z-tree (Fischbacher, 2007). Three sessions were run for each treatment with a total of 96 participants (30 in *CONTROL,* 33 in *ORDERED*, and 33 in *SEQUENTIAL*). Each subject participated only in one experimental treatment. At the beginning of each session participants were randomly allocated to a closed cubicle, where they could make their decisions in complete anonymity from the other participants. Sessions lasted on average 90 minutes and participants earned 46.17 CHF on average. At the time of the experiment, the exchange rate between the Swiss Franc and the American Dollar was 1 CHF=$1.23

The experimental instructions were adopted from FGF and given to subjects on paper. Instructions for Part 1 were handed out first. Participants were informed that there would be a second and third part to the experiment, but they had no prior knowledge of what the content of these parts would be. Instructions for the second part were not handed out until the end of first part, and similarly for the third part. In the instructions, the contribution table was presenting and explained in the *CONTROL* and *ORDERED* treatments. In the *SEQUENTIAL* treatment, two screens were presented to the participants and they did no know the sequence of others' contribution before playing the game.

After participants had read the instructions, they had to answer control questions which tested their understanding of the experiment. The experiment did not start until all participants had answered correctly the control questions. The procedures for the second and third part of the experiment were the same. At the end of the experiment the total amount of tokens earned by participants was converted to Swiss francs at the rate of 1 token = 0.6 CHF for the strategy method and the one-shot game, and, since each individual made 10 decisions in the third part, at the rate of 1 token = 0.06 CHF for the finitely-repeated game.

3 Results

This section is divided into four parts. The first part examines the impact of the different frames on the contribution schedules and, in particular, whether it affects the rate of non-monotonic contribution schedules. The second part investigates the predictive power of the contribution schedules under the different frames in the one-shot public-good game, while the third part does the same for the finitely-repeated game. The fourth part discusses other experimental findings such as the impact of the different frames on the distribution of cooperation preferences and the levels of contribution in the one-shot and finitely-repeated games.

3.1 Non-monotonic contribution schedules

All studies using the FGF method for eliciting cooperation preferences find that a non-trivial fraction of individuals provides non-monotonic contribution schedules. As mentioned, there are reasons to expect that there may be more (less) non-monotonic contribution schedules in the *SEQUENTIAL* (*ORDERED)* treatment compared to the *CONTROL* treatment.

Let G denote the average contribution of one's group members, $G \in \{0, 1, ...20\}$, and $g_i(G)$ the contribution of individual i given the average contribution of his peers. A contribution schedule is *weakly monotonic* if $g_i(G+1) \geq g_i(G)$ for all $G \in [0, 19]$, or $g_i(G+1) \leq g_i(G)$. A contribution schedule that does not satisfy either of these conditions is *non-monotonic.* For example, an individual who always contributes $g_i(G) = 0$ has a weakly monotonic schedule. A perfect conditional cooperator contributes $g_i(G) = G$ and has a strictly monotonic schedule. An individual who contributes $g_i(0) = 0$, $g_i(1) = 2$, $g_i(2) = 1$,and $g_i(3) = 4$ has a non-monotonic schedule. To have a measure of how "noisy" these schedules are, I will sometimes refer to the number of *switches* in the non-monotonic schedule of an individual. For example, in the previous example, individual i made one switch when he contributed $g_i(2) = 1$ (since before that he had an increasing contribution schedule), and a second switch when he contributed $g_i(3) = 4$.[8] The individual contribution schedules can be seen in Appendix A.

Result 1: *The proportion of individuals with non-monotonic contribution schedules is substantially higher in the* SEQUENTIAL *treatment than in the* CONTROL *treatment, and substantially lower in the* ORDERED *treatment than in the* CONTROL *treatment.*

[8]Such contribution schedules are difficult to reconcile with most models of social preferences as discussed in the previous section. Models of non-linear altruism could provide a justification for non-monotonic contribution schedules. According to these models an individual could contribute more when she believes other contribute low amounts, and less when others contribute more. These models however cannot account for contribution schedules exhibiting multiple "switching points". Most subjects who do not have a weakly monotonic schedule have multiple switching points (9 out of 10 in *ORDERED*, 25 out of 29 in *SEQUENTIAL* and 12 out of 15 in *CONTOL*).

SUPPORT: Figure 1 shows that the proportion of individuals with a non-monotonic contribution schedule is 50.0 percent in *CONTROL*, 30.3 percent in *ORDERED*, and 87.9 percent in *SEQUENTIAL*. A Chi-square test using each individual as an independent observation indicates that the rate of non-monotonic contribution schedules is statistically higher in *SEQUENTIAL* than in *CONTROL* ($p-$value=0.0011) and *ORDERED* ($p-$value=0.0001). While the rate is considerably higher in *CONTROL* than in the *ORDERED* (65 percent higher), the difference marginally fails to be significant at a conventional level ($p-$value=0.1292).[9]

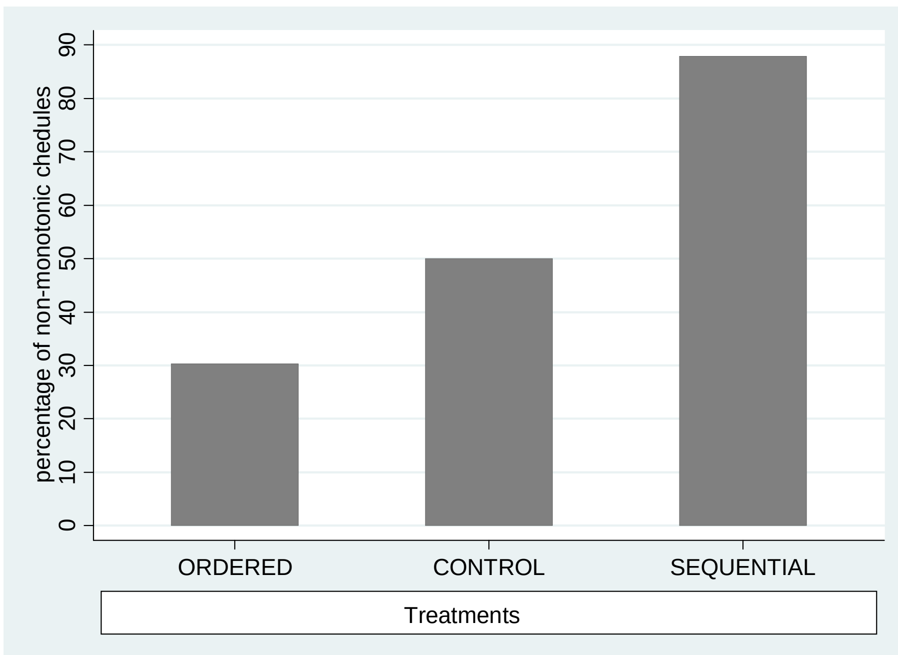

Figure 1: Percentage of subjects with non-monotonic contribution schedules

Result 2: *The average number of switches is significantly greater in the* SEQUENTIAL *than in the* CONTROL *treatment. The number of switches is not significantly different in the* ORDERED *and in the* CONTROL *treatment. Amongst the subjects with non-monotonic contribution schedules, the average number of switches does not differ significantly across treatments.*

SUPPORT: The average number of switches is 2.0 in the *CONTROL* treatment, 3.5 in *SEQUENTIAL* and 1.2 in *ORDERED*. Since there are more than two treatments, I first report the results

[9] For completeness, I report that the rate of non-monotonic contribution schedules is significantly higher in *SEQUENTIAL* than in *ORDERED* ($p-$value=0.0001).

from a two-tailed Kruskal-Wallis test. I then proceed to pair-wise treatment comparisons only if the Kruskal-Wallis indicates significant differences across treatments. The Kruskal-Wallis test reveals a significant difference across treatments ($p-$value=0.0001). Using a two-tailed Mann-Whitney test with each individual as an independent observation, I find that the difference is significant between *CONTROL* and *SEQUENTIAL* and ($p-$value=0.0084). The difference between *CONTROL* and *ORDERED* narrowly misses the 10-percent level of significance ($p-$value=0.1084).[10] Nevertheless, the number of switches amongst individuals with non-monotonic schedules is similar across treatments (3.9 in *ORDERED*, 4.0 in *CONTROL* and 4.0 in *SEQUENTIAL*) and not significantly different (Kruskal-Wallis; $p-$value=0.8776). This indicates that the difference in the number of switches on average is due to the higher percentage of non-monotonic contribution schedules in *SEQUENTIAL* (see Result 1).

The greater extent of non-monotonic schedules in *SEQUENTIAL* may be partly attributed to the fact that individuals cannot revise their choices once made. However, half of the participants also fail to report a weakly monotonic contribution schedule even in *CONTROL* where revisions are possible. Given the impact of the frames on the monotonicity of contribution schedules, the next subsection investigates whether and how the different frames also affect the predictive power in the one-shot public-good game.

3.2 Predictive power of schedules in the one-shot public-good game

A natural way to investigate the predictive power of the contribution schedules is to examine whether the actual contributions of participants in the one-shot game deviate from those in the contribution schedules. Recall that participants were asked to state how much they believed their peers would contribute on average in the one-shot game. Following Fischbacher and Gächter (2010), I use this belief and the contribution schedule to obtain a prediction about how much an individual will contribute in the one-shot game. For example, if someone believes that his peers will contribute on average 5, the *predicted contribution* is obtained by looking at the contribution the individual stated he would make if the others contributed 5 on average in the contribution schedule.

Result 3: *On average, contribution schedules are accurate predictors of contributions in the one-shot game only in the* ORDERED *treatment.*

SUPPORT: Let *Deviation* be the difference between a subject's actual and predicted contribution. Figure 2 presents the distribution of *Deviation* in each treatment. As can be easily seen, relative

[10]The difference between *SEQUENTIAL* and *ORDERED* is highly significant ($p-$value=0.0000)

to the *CONTROL* treatment, more subjects deviate from the predicted contribution in the *SEQUENTIAL* treatment, and less in the *ORDERED* treatment. The average deviation is 1.4 tokens in *CONTROL,* 1.8 tokens in *SEQUENTIAL* and 0.2 in the *ORDERED*. A two-tailed Wilcoxon signed-rank test using each individual as an independent observation indicates that the deviation is not significantly different from 0 in the *ORDERED* condition ($p-$value=0.8266), but it is in the *CONTROL* ($p-$value=0.0653) and in the *SEQUENTIAL* treatment ($p-$value=0.0047).[11]

The average deviation from the predicted contribution is one obvious indicator for evaluating the predictive power of contribution schedules. In this case, however, it masks the fact that the schedules predict perfectly the contribution of nearly 50 percent of subjects in the *CONTROL* and *ORDERED* treatments. Figure 2 shows that the relative efficacy of the schedules in *ORDERED* is due to the (roughly) equal number of positive and negative deviations from the predicted contribution. For this reason, next, I investigate the predictive power of the contribution schedules using a different measure.

[11] This difference is not statistically significant between *SEQUENTIAL* and *CONTROL* ($p-$value=0.3300). Despite the large difference, a Mann-Whitney test fails to reject the hypothesis that average devation is the same in *ORDERED* and *CONTROL* ($p-$value=0.2406). The difference in average devation between *ORDERED* and *SEQUENTIAL* is statistically significant ($p-$value=0.0375).

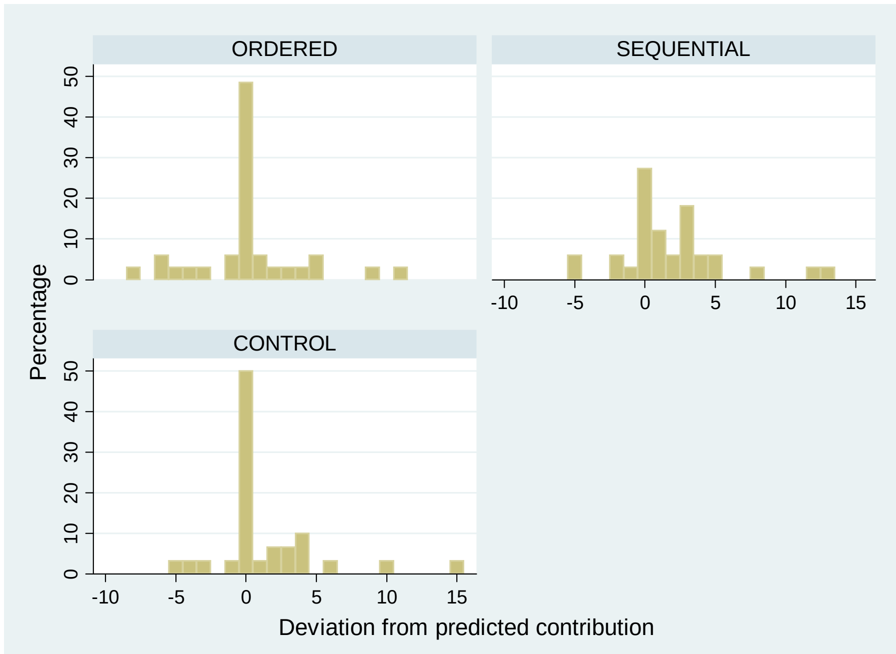

Figure 2: Distribution of deviation from the predicted contribution in the one-shot game.

Result 4: *Relative to the* CONTROL *treatment, the probability an individual's contribution differs from their predicted contribution is higher in* SEQUENTIAL, *and (insignificantly) lower in* ORDERED. *This probability of deviation is higher for individuals with noisy contribution schedules and for those with high beliefs about the contribution of their peers.*

SUPPORT: Table 1 reports the results from a regression analysis investigating the determinants of an individual's deviation from their predicted contribution. The dependent variable is a binary variable taking the value of 1 if $Deviation \neq 0$ and the value of 0 if $Deviation = 0$. Model 1 includes only treatment dummies as regressors (*CONTROL* being the omitted category). The regression shows that the probability of deviating from the predicted contribution is 22.6 percent higher in *SEQUENTIAL* than in *CONTROL* ($p-$value=0.050). The difference between *CONTROL* and *ORDERED* is small (1.5 percent) and statistically insignificant ($p-$value=0.904). Model 2 includes the variable "*Switcher*" as an explanatory variable. *Switcher* is a dummy variable taking the value of 1 if the contribution schedule of a particular individual includes more than 1 switches.[12]

[12] The rationale for this is that, as mentioned earlier, none of the standard models of social preference can account for more than one switches. Note that "triangle" contributors have one switch in their contribution schedules.

Model 2 indicates that the probability a "switcher" deviates from his predicted contribution is 31.38 percent higher than that of a non-switcher ($p-$value=0.003). The fact that the size of the *SEQUENTIAL* coefficient is approximately halved and is no longer significantly different from zero ($p-$value=0.326) indicates that the difference across the two treatments is mainly due to the higher number of individuals with non-monotonic contribution schedules in this treatment. To test this explanation further, I ran a regression which in addition to the regressors in Model 2, included interaction terms between the treatment and switcher dummies (not reported). The only significant variable in this regression is the *Switcher* variable (marg. effect: 36.27 percent; $p-$value=0.047). The coefficient for *SEQUENTIAL* is slightly lower than in Model 2 and remains statistically insigificant (marg. effect: 10.78 percent; $p-$value=0.591). The fact that the interaction terms are insignificant indicates the switchers are as likely to deviate from the predicted contribution in all treatments and corroborates Result 2. Model 3 adds an individual's *Belief* as a regressor. The results indicate that the higher the belief of an individual about the average contribution of his peers, the higher is the probability they deviate from their predicted contribution (marginal effect: 3.22 percent; $p-$value=0.005). This seems intuitive. For example, while an individual may have stated that she would contribute 15 if she knew *for sure* that others did the same (strategy method), in the one-shot game, she cannot be sure whether this will be the case. Therefore, she may be more likely to deviate from her predicted contribution than if she believed others would contribute 5. The reason is that participants, on average, contribute less than their beliefs. Therefore, the extent of the deviation is lower when beliefs are low. A similar finding regarding beliefs is also reported in Fischbacher and and Gächter (2010). Finally, Model 4 replaces the *Switcher* dummy variable with the total number of switches in an individual's contribution schedule. As can be seen, the higher the number of switches in a contribution schedule, the higher is the probability that an individual deviates from his predicted contribution ($p-$value=0.015).[13] The marginal effect shows that an additional switch in the contribution schedule increases the probability of deviating from the predicted contribution by 6.21 percent.

3.3 Predictive power of schedules in the finitely-repeated public-good game

The approach for evaluating the predictive power of the contribution schedules in the finitely-repeated game is the same as in the previous section. In each of the ten periods, I estimate an

[13]The results are qualitatively unaffected if we use dummies to control for the number of switches instead of the total number of switches.

Model	1	2	3	4
ORDERED	0.0147	0.0588	0.0678	0.0872
	(0.1227)	(0.1252)	(0.1299)	(0.1303)
SEQUENTIAL	0.2263**	0.1273	0.1219	0.1414
	(0.1153)	(0.1296)	(0.1336)	(0.1300)
Switcher		0.3138***	0.2933***	
		(0.1041)	(0.1089)	
Beliefs			0.0322***	0.0308***
			(0.0113)	(0.0113)
Totalswitch				0.0621**
				(0.0255)
Observations	96	96	96	96

Table 1: Probability of deviating from predicted contribution in the one-shot game. Probit regression. Entries are marginal effects. Standard errors are in parentheses. *p-value<0.1,**p-value<0.05,***p-value<0.01

individual's *Deviation* by comparing their actual to their predicted contribution.[14]

Result 5: *Contribution schedules accurately predict contributions in the finitely-repeated game in the* CONTROL *and the* ORDERED *treatments, but not in the* SEQUENTIAL *treatment.*

SUPPORT: Figure 3 presents the distribution of *Deviation* in each treatment. As can be seen, more subjects appear to deviate from their predicted contribution in the *SEQUENTIAL* than in the *CONTROL* treatment. The *CONTROL* and *ORDERED* treatments give similar results. The average deviation across the 10 periods is 0.3 in *CONTROL,* -0.6 in the *ORDERED*, and 1.4 in *SEQUENTIAL*. To control for the panel nature of the data in the third part of the experiment, I ran a linear regression controlling for random effects at the session level. The average deviation is significantly different from zero in *SEQUENTIAL* ($p-$value=0.003), but not in *CONTROL* and *ORDERED* ($p-$value=0.637 and 0.322, respectively).[15] These results are robust if I evaluate behavior at different points of part 3, such as in period 1 and the first five periods.

[14] Note that participants received feedback about the contributions of their peers and their private earnings at the end of each period, but also at the start of the first period (i.e., regarding decisions in the one-shot game).

[15] Average deviation is signficantly different between *SEQUENTIAL* and *CONTROL* ($p-$value=0.081), and between *ORDERED* and *SEQUENTIAL* ($p-$value=0.005). Average devation is not significantly different between *ORDERED* and *CONTROL* ($p-$value=0.302).

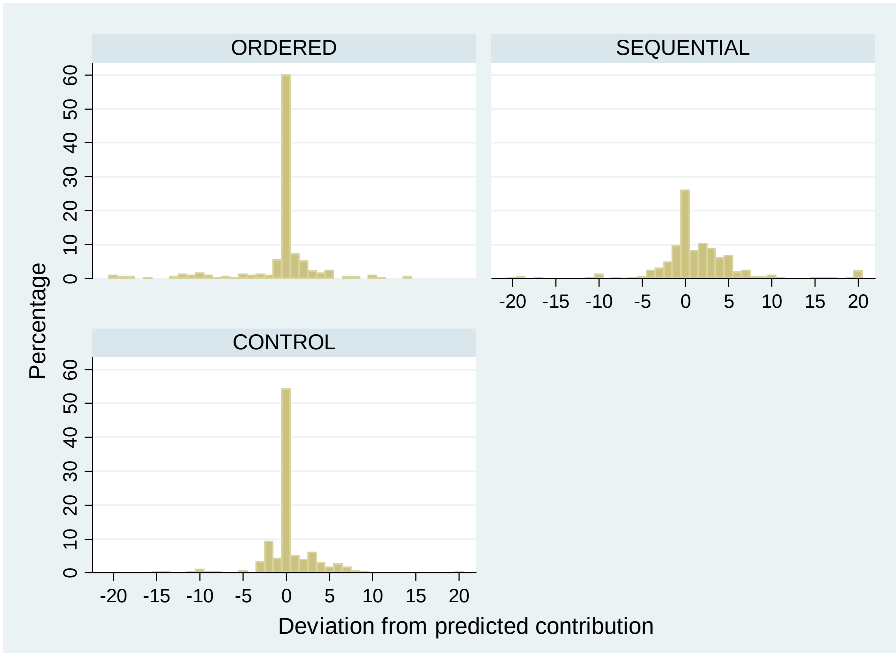

Figure 3: Distribution of deviation from the predicted contribution
in the finitely-repeated game

Result 6: *The probability an individual's contribution differs from their predicted contribution in the finitely-repeated game is higher in* SEQUENTIAL *than in* CONTROL. *The difference between* CONTROL and ORDERED *is not statistically significant. The probability of deviating from the predicted contribution is higher for individuals with noisy contribution schedules and with high beliefs about the contribution of their peers.*

SUPPORT: Table 2 reports the results from a regression analysis investigating the determinants of an individual's deviation from their predicted contribution. The dependent variable, as in the previous subsection, is a binary variable taking the value of 1 if $Deviation \neq 0$ and the value of 0 if $Deviation = 0$ for a given subject in a given period. Given the interdependence of contributions at the session level, the model controls for random effects at the session level. The logic of the empirical investigation is the same as in the previous section for the one-shot game, building the model up gradually.

The results in Table 2 are qualitatively the same as those in Table 1 for the one-shot game. The regression shows that the probability of a subject deviating from her predicting contribution is 29.5 percent higher in the *SEQUENTIAL* than in the *CONTROL* treatment ($p-$value<0.001).

Contributions in the *ORDERED* treatment are 4.9 percent less likely to differ from the predicted contribution than in the *CONTROL* treatment, but the difference is far from being statistically significant ($p-$value=0.427). Model 2 shows that a "*Switcher*", that is, an individual with noisy contribution schedule is 28.2 percent more likely to deviate from his predicted contribution ($p-$value<0.001). However, unlike in the one-shot game, the coefficient for *SEQUENTIAL* remains significant in Model 2 ($p-$value=0.002), although the coefficient drops from 28.2 to 21.1 percent. Model 3 controls for an individual's *Belief* about other's contribution. Similar to the one-shot game, the higher the belief of an individual about the average contribution of her peers, the higher is the probability she deviates from her predicted contribution (marg. effect: 4.7 percent; ($p-$value<0.001). Note that the coefficient for *SEQUENTIAL* is no longer significant in Model 3. This indicates that the higher rate of deviations in this treatment is mostly due to the higher beliefs about the contributions of others.[16] Model 4 replaces the *Switcher* variable with the total number of switches in an individual's contribution schedule, and adds a (linear) control for time effects. The results indicate that the higher the number of switches in a contribution schedule, the higher is the probability that an individual deviates from his predicted contribution ($p-$value<0.001). The marginal effect shows that, similar to the one-shot game, an additional switch in the contribution schedule increases the probability of deviating from the predicted contribution by 6 percent. The probability of deviating from one's predicted contribution decreases by 1 percent in every period. The reason is that, as in all public-good experiments, contributions decline over time. As we will see in the next subsection, most people contribute small amounts in response to low contribution by their peers.

3.4 Cooperation preferences, contribution levels and beliefs

So far, the analysis has focused on how the different frames affect the monotonicity and predictive power of the contribution schedules. In this section, I investigate the impact of the different frames on the levels of contribution in the three parts of the experiment and subjects' beliefs. Before doing this, however, I will examine how the frames impact the different types of cooperation preferences seen in previous studies. Fischbacher, Gächter and Fehr (2001) proposed a classification of the

[16] To test this explanation, I ran a regression separately for each treatment with the sole regressors being *Switcher* and *Belief.* The latter is always statistically significant, while the former is signficant in all treatments except in the *SEQUENTIAL.* A closer inspection at the data indicates the both switchers and non-switchers have substantially higher levels of beliefs in this treatment. As a result, both are about 75 percent likely to deviate from their predicted contribution - a rate which is considerably higher than in the other treatments. I discuss the impact of framing on contributions and beliefs in more detail in the next subsection.

Model	1	2	3	4
ORDERED	-0.0492	-0.0127	-0.03480	-0.0154
	(0.0620)	(0.0725)	(0.0576)	(0.0592)
SEQUENTIAL	0.2950***	0.2112***	0.0646	0.0717
	(0.0558)	(0.0690)	(0.0610)	(0.0616)
Switcher		0.2822***	0.2649***	
		(0.0346)	0.0356	
Beliefs			0.0474***	0.0486***
			(0.0059)	(0.0063)
Totalswitch				0.0603***
				(0.0079)
Period				0.0107*
				(0.0063)
Observations	960	960	960	960

Table 2: Probability of deviating from predicted contribution in the finitely-repeated game. Probit regression with random effects at the session level. Entries are marginal effects. Standard errors are in parentheses. *p-value<0.1,**p-value<0.05,***p-value<0.01

different patterns observed in the contribution schedules. In particular, individuals that always contribute zero in the strategy method, irrespective of the contribution of their peers, are classified as "free riders". Individuals who have a contribution schedule with either a weakly monotonic pattern with at least one increase or a positive Spearman rank correlation significant at the 1-percent level are classified as "conditional cooperators".[17] "Triangle contributors" are participants who have a significantly increasing schedule up to some maximum and a significantly decreasing schedule thereafter, using again as a criterion the Spearman rank test at the 1-percent level of significance. Participants that could not be classified in one of the above categories are classified as "others" or "confused".

Treatments/types in percent	CONTROL	ORDERED	SEQUENTIAL
Free Riders	30.00	36.36	3.03
Conditional Cooperators	43.33	39.39	78.79
Triangles	3.33	3.03	0.00
Others	23.33	21.21	18.18

Table 3 : Type classification in percentage

[17] Note that conditional cooperators can have non-monotonic schedules and, indeed, some of them did.

Result 7: *The sequential frame has a pronounced impact on the elicited contribution schedules relative to the CONTROL treatment. In contrast, the ordering of the others' contribution does not affect the elicited contribution schedules.*

SUPPORT: Table 3 presents the results of the FGF classification method in each treatment. While the distribution of types appears to be similar in *CONTROL* and *ORDERED,* it is strikingly different in *SEQUENTIAL.* As can be seen, only 1 out of 33 participants (3 percent) can be classified as selfish in the *SEQUENTIAL* treatment, compared to 12 out of 33 in the *ORDERED* treatment (36.4 percent), and 9 out of 30 in the *CONTROL* treatment (30 percent). Similarly, 43 percent of individuals are classified as conditional cooperators in the *CONTROL* treatment, 39.4 percent in *ORDERED,* and 78.8 percent in *SEQUENTIAL.*[18] A Fischer's exact test rejects the hypothesis that the distribution of types is the same across the three treatments ($p-$value=0.003). Pairwise Fischer exact tests reveal that this difference is due to the *SEQUENTIAL* treatment. In particular, the difference between *CONTROL* and *SEQUENTIAL* is significant ($p-$value=0.004), while that between *ORDERED* and *CONTROL* is not ($p-$value=0.947).

[18] It is also worthwhile pointing out that only 3 of the 13 individuals classified as conditional cooperators in *ORDERED* have a non-monotonic contribution schedule. In contrast, 8 of 13 conditional cooperators in *CONTROL,* and 24 of the 26 in *SEQUENTIAL* have non-monotonic schedules. The proportion of conditional cooperators with non-monotonic schedules, relative to the *CONTROL* treatment, is lower in *ORDERED* ($p-$value=0.0183) and higher in *SEQUENTIAL* ($p-$value=0.03). Conditional cooperators are switching more often on average in *SEQUENTIAL* (3.0 switches) and *CONTROL* (2.1 switches) compared to *ORDERED* (0.5 switches). A Mann-Whitney test indicates that the difference between *ORDERED* vs *CONTROL* is statistically significant ($p-$value=0.0398), while that between *CONTROL* and *SEQUENTIAL* is not ($p-$value=0.1239).

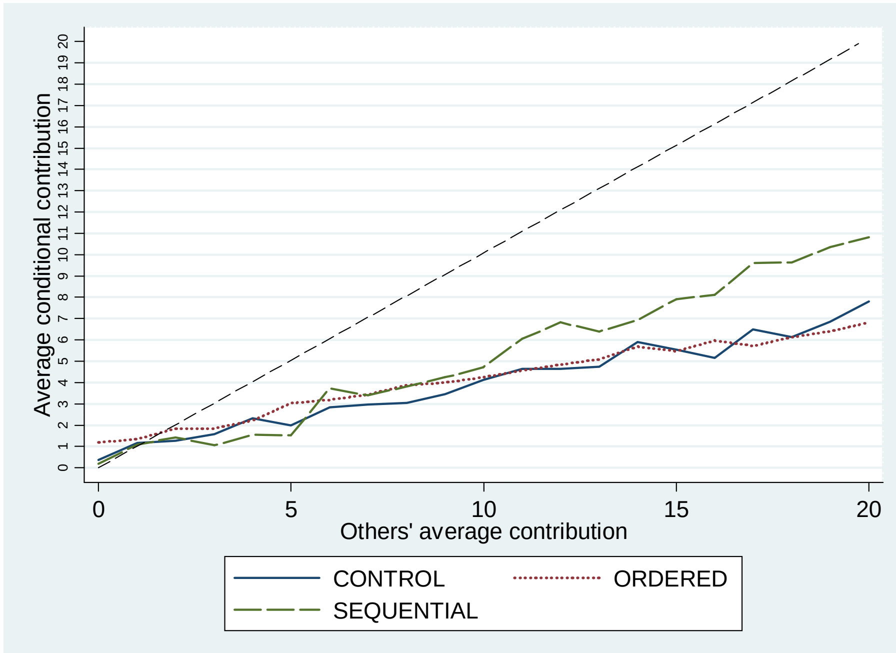

Figure 4: Average conditional contribution in strategy method

Additional support for Result 7 can be found in Figure 4 and Table 4.[19] Figure 4 presents the average conditional contribution in the first part of the experiment. Similar to previous experiments, the average conditional contribution is monotonically increasing and lies below the 45-degree line in all treatments. However, as can be seen, conditional contributions tend to be higher in *SEQUENTIAL*, than in the other two treatments. The results of a linear regression with individual random effects reported in Table 4 confirm that the average conditional contribution differs significantly in *SEQUENTIAL.* In particular, the slope of the conditional contribution is higher by 20.9 degrees in *SEQUENTIAL* than in *CONTROL.* This difference is non-trivial and statistically significant ($p-$value<0.001). It implies that for every additional token contributed to the public account by one's peers, the contribution will be 0.209 higher than in the *CONTROL.* The difference in slopes between *CONTROL* and *ORDERED* is smaller (5.2 degrees) and narrowly misses the 10-percent level of significance ($p-$value$=0.101$).

Result 8: *By and large, beliefs and unconditional contributions are not significantly different*

[19]Figure 4 and Table 4 below excludes subject 1202 who gave 20 when others gave 0 and had a Spearman correlation coefficient of -1

Model	1
Others' contribution	0.3320***
	(0.2296)
ORDERED	0.7085
	(1.0552)
SEQUENTIAL	-0.0320
	(1.0474)
ORDERED * Others' contribution	-0.0524
	(0.0319)
SEQUENTIAL * Others' contribution	0.2087***
	(0.0317)
Constant	0.6331
	0.7580
Observations	95

Table 3: Determinants of conditional contribution in the strategy method. Linear regression with individual random effects. *p-value<0.1,**p-value<0.05,***p-value<0.01

across treatments in the first and second part of the experiment. Contributions are higher in the finitely-repeated game in SEQUENTIAL, due to higher beliefs about the contributions of others.

SUPPORT: [First part of the experiment] The average belief regarding others' average (unconditional) contribution in the first part of the experiment is 7.16 (*CONTROL*), 5 (*ORDERED*), and 8 (*SEQUENTIAL*). A Kruskal-Wallis test rejects the hypothesis that beliefs are the same across treatments ($p-$value=0.0408). Using a two-tailed Mann-Whitney test with each individual as an independent observation, the difference between *CONTROL* and *ORDERED* is marginally statistically significant ($p-$value=0.0999), while that between *CONTROL* and *SEQUENTIAL* is not ($p-$value=0.6265). In contrast, a Kruskal-Wallis test fails to reject the hypothesis that subjects' unconditional contributions (6.23, 4.42, 6.57, in *CONTROL, ORDERED* and *SEQUENTIAL,* respectively) are the same across treatments in the first part of the experiment ($p-$value=0.1413).

[Second part of the experiment] A Kruskal-Wallis test fails to reject the hypothesis that subjects' beliefs (6.03, 5.81, 6.72, in *CONTROL, ORDERED* and *SEQUENTIAL,* respectively) are the same across treatments ($p-$value=0.6003). While the test rejects the same hypothesis for contributions (4.93, 3.85, 5.82, in *CONTROL, ORDERED* and *SEQUENTIAL,* respectively; $p-$value=0.0703), a two-tailed Mann-Whitney test with each individual as an independent observation indicates that neither the difference between *CONTROL* and *ORDERED* is statistically significant ($p-$value=0.3170) nor is that between *CONTROL* and *SEQUENTIAL* ($p-$value=0.2714).

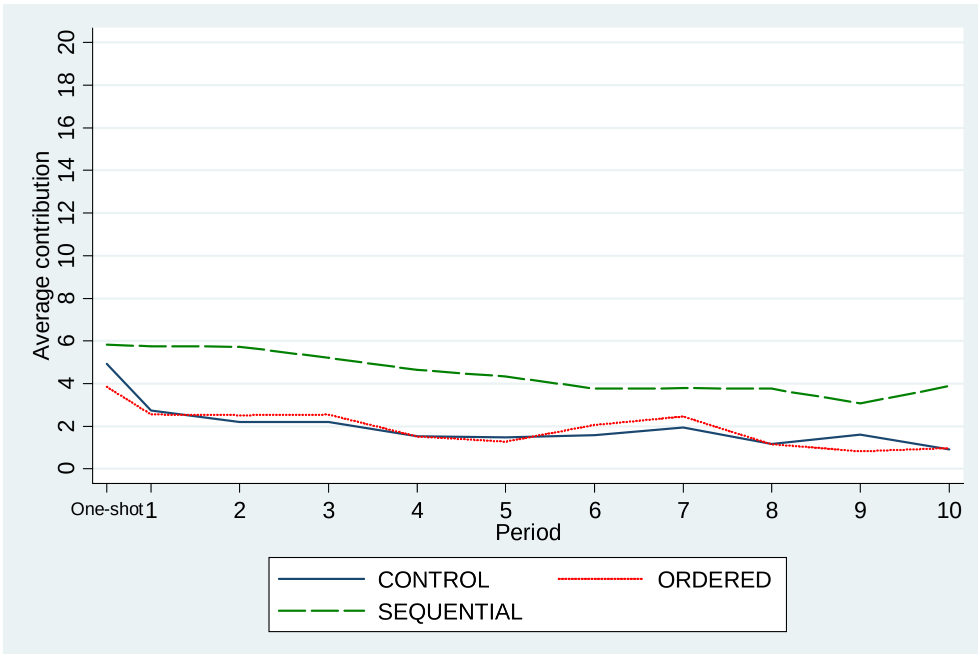

Figure 5: Average contribution over time

[Third part of the experiment] As can be seen in Figure 5, while contributions appear to be very similar across all periods in *CONTROL* and *ORDERED,* contributions tend to be higher in the *SEQUENTIAL* treatment than in the *CONTROL*. In particular, average contribution is 1.7 in *CONTROL*, 1.8 in *ORDERED* and 4.3 in *SEQUENTIAL.* Similarly, on average, beliefs are higher in *SEQUENTIAL* (5.5), than in *CONTROL* (2.12) and *ORDERED* (2.42). This seems surprising given that beliefs and contributions did not differ significantly in the first two parts of the experiment (Result 7). The difference appears already in the first period of the third part where beliefs in *SEQUENTIAL* are 6.2, 3.8 in *CONTROL,* and 4.8 in *ORDERED.* Given the panel nature of the data and the use of random matching in this part of the experiment, to compare behavior across treatments Table 5 presents the results from a linear regression with random effects at the session level. Model 1 illustrates that the difference between *CONTROL* and *SEQUENTIAL* is statistically significant ($p-$value=0.005), but not that between *CONTROL* and *ORDERED* ($p-$value=0.945). The addition of subjects' beliefs as an explanatory variable in Model 2 indicates that once I control for beliefs, the difference between *CONTROL* and *SEQUENTIAL* is no longer significant ($p-$value=0.921). The coefficient of subjects' beliefs is highly significant ($p-$value<0.001) and indicates that a one unit increase in beliefs, increases the con-

Model	1	2	3
ORDERED	0.0708	-0.1913	-0.01871
	(1.0328)	(0.2533)	(0.3622)
SEQUENTIAL	2.8676**	-0.0276	-0.2773
	(1.0328)	(0.2780)	(0.4298)
Period	-0.2035***	0.0201	0.0167
	0.0433	(0.0369)	(0.0374)
Beliefs		0.8011***	0.7678***
		(0.0342)	(0.1094)
Beliefs * ORDERED			0.0025
			(0.1190)
Beliefs * SEQUENTIAL			0.0660
			(0.1184)
Constant	2.8173***	-0.0793	0.0099
	(0.7697)	(0.2961)	(0.3890)
Observations	960	960	960

Table 4: Determinants of contributions in the finitely-repeated game. Linear regression with session random effects. *p-value<0.1,**p-value<0.05,***p-value<0.01

tribution of an individual by 0.80 tokens. Finally, Model 3 shows that the relationship between beliefs and contributions is similar across treatments. This implies that higher beliefs about others' contribution drive the higher levels of contribution in *SEQUENTIAL.* In Appendix B, I provide additional evidence from a regression analysis of subjects' beliefs. I show that the higher beliefs in *SEQUENTIAL* are due to a stronger relationship between contributions in parts 1 and 2 (i.e., the feedback subjects receive at the start of part 3), and beliefs. It is difficult to explain why the sequential frame has the effect on belief formation.

4 Discussion

The aim of the experiment was to evaluate the sensitivity of cooperation preferences to changes in the frame which have been recently shown to affect the elicitation of (risk) preferences (Levy-Garboua et al., 2012). In particular, using the method of Fischbacher, Gächter and Fehr (2001; FGF) for eliciting cooperation preferences, I varied (*i*) the order in which others' contributions appeared in the experiment, and (*ii*) whether these contributions were presented simultaneously or in sequence. In addition, the experiment aimed to evaluate the predictive power of cooperation preferences in a one-shot and a finitely-repeated public-good game. In general, I found that the order in which the contributions of others was presented had no impact on the elicited preferences and their predictive power. However, presenting the contributions of others in a sequence had a significant effect on the elicited preferences and reduced their predictive power. In this sense, my findings are similar to those of Levy-Garboua et al. (2012) who found that risk preferences are affected more by changes in whether the options are presented sequentially or simultaneously, rather than the order in which the options are presented. Overall, elicited preferences are more accurate at predicting behavior when others' contributions are presented simultaneously and in ascending order, like in Fischbacher, Gächter and Fehr (2001).

What could explain the impact of the sequential frame in our experiment? One explanation for the number of non-monotonic schedules in *SEQUENTIAL* may be that individuals cannot revise their choices once made. While this explanation can partly account for the level of noise in the contribution schedules, it cannot account for the much higher levels of conditional cooperation and near absence of free riders seen in this treatment. An explanation for this finding may be that individuals wish to maintain a positive self-image. Gneezy et al. (2011) found that donations to charity are more likely to happen after people lie or fail to return money they had received by mistake. The authors discuss the concept of "conscience accounting" which means that people try to compensate "bad" activities to protect their identity and self-image". It seems possible that making 21 consecutive decisions not to contribute to the public account may be more damaging for one's self image than submitting once a table with zero contributions.

An issue which may be interesting for future study is when cooperation preferences are elicited. Fischbacher and Gächter (2010) ran experiments with the FGF strategy method either at the start or the end of the experiment to evaluate whether the timing of the elicitation affected contributions in a finitely-repeated game. They found that this was not the case and that contributions were the same when cooperation preferences were elicited at the start and the end of the experiment. This finding is the reason I elicited preferences only at the start of the experiment. However, the finding that beliefs and contributions in the finitely-repeated game are higher in *SEQUENTIAL* suggests that eliciting cooperation preferences at the start of the experiment may influence outcomes, at least under some frames. Therefore, it may be useful for future studies to randomize when the strategy method is administered.

Finally, given the findings from the present experiment, I believe that the FGF method could be used to investigate whether other kinds of frames that have been shown to affect contributions in public-good games, such as the warm-glow/cold-prickle effect of Andreoni (1995) and the labelling of the game or strategies (e.g., Ellingsen et al., 2012; Kay and Ross, 2003; Liberman et al., 2004; Ross and Ward, 1996) affects only subjects' beliefs, as suggested in previous articles (Dufwenberg et al. 2011; Ellingsen et al., 2012; Nikiforakis, 2010) or also cooperation preferences.

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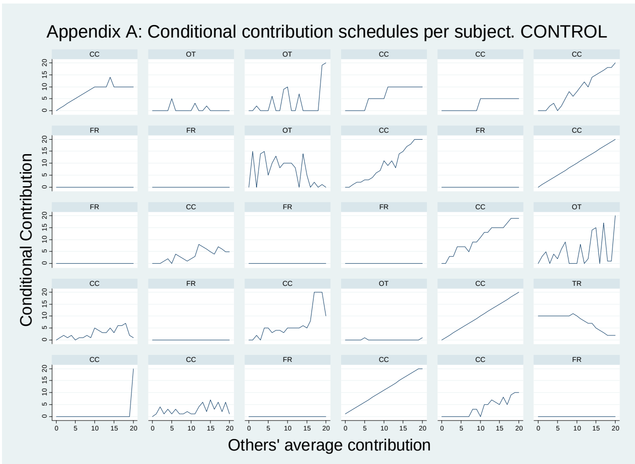
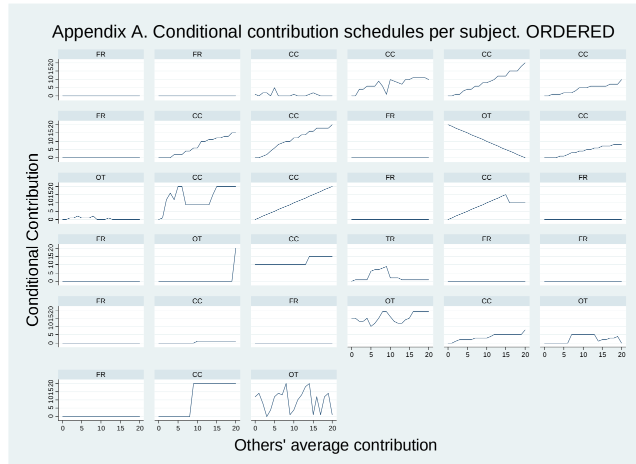
Appendix A: Individual contribution schedules





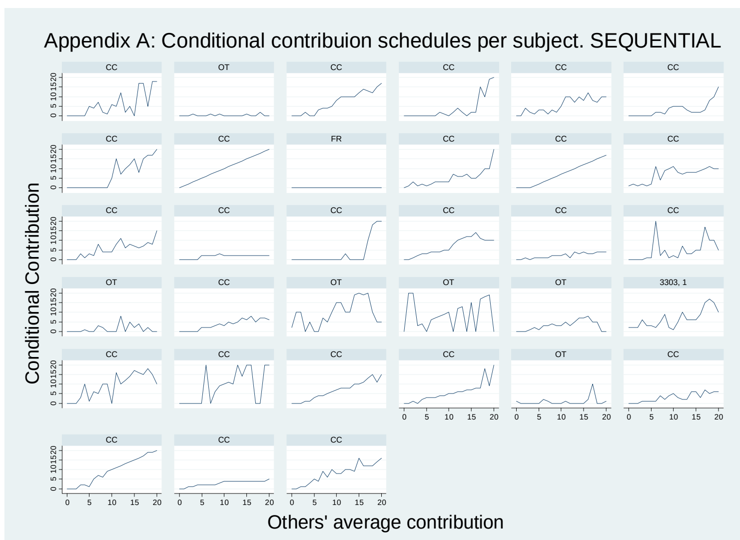


Figure A1: Individual contribution schedules
The letters on top of each subgraph indicate how that particular individual was classified (CC: conditional cooperator; FR: free rider; TR: triangle; OT: other)

Appendix B: Determinants of beliefs in the finitely-repeated game

Treatments	CONTROL	ORDERED	SEQUENTIAL
Period	-0.2375***	-0.3487***	-0.2477***
	(0.0302)	(0.0606)	(0.0570)
Average Contribution in SM	0.0912***	0.0003	0.2371***
	(0.0207)	(0.0690)	(0.0639)
Average Contribution in One-Shot	0.1343***	0.2927***	0.4484***
	(0.0517)	(0.0524)	(0.0582)
Constant	2.1949***	3.2171***	2.6695***
	(0.3144)	(0.0504)	(0.5111)
Observations	300	330	330

Linear regression with random effects at the session level.

Average Contribution in SM refers to the average contribution in the strategy method

Average Contribution in One-Shot refers to the average contribution in the one-shot game

*p<0.1,**p<0.05,***p<0.01